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Putting the Aero Back Into Aeroelasticity

William G. Bousman

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Abstract

The lack of progress in understanding the physics of rotorcraft loads and vibration over the last 30 years is addressed in this paper. As befits this extraordinarily difficult problem, the reasons for the lack of progress are complicated and difficult to ascertain. It is proposed here that the difficulty lies within at least three areas: 1) a loss of perspective as to what are the key factors in rotor loads and vibration, 2) the overlooking of serious unsolved problems in the field, and 3) cultural barriers that impede progress. Some criteria are suggested for future research to provide a more concentrated focus on the problem.

Introduction

The dynamics community has necessarily focused on the twin issues of rotor loads and vibration since the beginning of the industry. There has been gradual progress in recent decades in our ability to accommodate loads and to reduce the effects of vibration, but it is the premise of this paper that our understanding of the physics of the aerodynamic loading and response of a rotor has not shown a significant improvement in the last thirty or so years. It was well known in the 1950s that a uniform wake was not suitable for the accurate prediction of rotor aerodynamic loading. Piziali and Du Waldt (Ref. 1) demonstrated a substantial advance in prediction capability by incorporating a prescribed wake in their analysis. Landgrebe (Ref. 2) showed that it was possible to model the wake as a distorted or free wake. By the early 1970s it appeared that the representation of the basic physics of the rotor was in place. The 1973 AGARD "Specialists Meeting on Helicopter Rotor Loads Prediction Methods" in Milan in some ways represents a watershed in the development of analytical methods. Loewy (Ref. 3) saw the progress that had been made when he said, "Instead of running into unexpectedly high loads almost everywhere the first time the full flight envelope is explored, we now only run into them occasionally, at some extreme flight condition." Piziali (Ref. 4), in his commentary on the conference, was less sanguine and expressed his opinion that the advances of the past decade had been in the scope of the analyses, but not in their accuracy. He felt that the structural problem was well in hand, but that further advances would require improvements in the aerodynamic models.

The past decades since the Milan meeting have been largely marked by incremental improvements in the prediction of rotor loading, although there are few cases where these "improvements" have been demonstrated clearly and systematically. In the area of rotor loads calculation the critical design loads that are computed in maneuvers are sometimes of the correct size, but they sometimes have the incorrect phase, as shown by Sopher and Duh (Ref. 5) for a Sea Hawk. In one sense these loads are suitable for design, but the phase errors show a deficiency in the modeling of the physics that offers a cautionary note. For vibration, the present comparison of analytical methods with flight test data is not satisfactory as recently shown by Hansford and Vorvald (Ref. 6). In particular, they conclude that the structural modeling capability represented by the recently-developed finite element methods shows no advancement in predictive accuracy over classical modal techniques. The problem of prediction has perhaps been best characterized by Johnson (Ref. 7) who said, "For a good prediction of loads it is necessary to do everything right, all of the time. With current technology it is possible to do some of the things right, some of the time."

The purpose of the present paper is threefold. First, I will attempt to put the loads and vibration problems in their proper perspective and at the same time attempt to

demythologize some unfortunate trends of the past decade. Second, I will offer some sample problems in loads and vibration that are presently unsolved and I will suggest that real progress must be made on these problems before there is any hope of advancing the state of the art. Third, I will discuss a number of cultural problems related to progress in the sciences and offer some modest suggestions for improvement in these areas.

A Search for Perspective

The problems of loads and vibration have always been a part of helicopter development and in this sense have been at the forefront of all efforts by dynamicists in the industry. By necessity, the primary motivation has always been to solve or reduce the problems that crop up during the helicopter's development phase. If at the same time an advance in our understanding of the basic physics is achieved then this is well and good, but that understanding has never been the primary objective. In the last 20 years the contributions of academia to these problems have become more important and in a sense the academic perspective is reversed from the industrial perspective. That is, the academic practitioner needs to understand the physics, even if this does not lead immediately to a practical result, while the industrial practitioner must achieve a practical result even if the physics is not fully understood. The best of all possible worlds is when a balance is achieved between these conflicting objectives, but sometimes things go awry. In some sense the dynamics community has lost its perspective on a number of topics in recent years and I will offer my view on what is needed to return to center in three areas. First, in our attempts to find the "silver bullet" that will solve all our problems we tend to oversimplify the task at hand and suggest that there is really only one best approach. I believe this approach will always be fruitless and I attempt to illustrate this fruitlessness with an example. Second, the physics of vibration are enormously complex and we need to understand the differences as well as the similarities in different flight regimes. Third, descending flight, which is so important for blade-vortex interaction (BVI) noise, is a benign environment for vibration and new vibration control approaches should not be demonstrated for these conditions but, rather, for the low-speed and high-speed vibration regimes.

Which Is the Silver Bullet–Rotor Wake or Blade Elasticity?

When analytical methods become sufficiently developed so that they can consistently and accurately predict the loads and vibration for a new rotorcraft, then it will be possible to determine the relative importance of various aspects of the modeling problem. Until that time comes we are reduced to endless arguments over whether the rotor wake is more important than blade aeroelasticity, or whether torsional deformations are more important than bending deformations, or whether the trim solution is the key to the problem.

In 1990 a sample problem of a Puma rotor in high-speed flight was examined using a variety of analytical methods in a stepwise fashion (Ref. 8). Ten calculations were established, as shown in Table 1, progressing from the simplest possible representation of a rotor, to the full calculation with all capabilities of the analysis exercised. The first calculation, Case 1, used a uniform wake (no harmonic variation), linear airfoil section properties, no unsteady aerodynamics, no radial flow corrections, and the blade dynamics were represented by a rigid, hinged blade. The rotor configuration was made progressively more complex in a stepwise fashion. For Case 3, a prescribed wake model replaced the uniform wake. For Case 5, nonlinear airfoil properties were used, based on a set of equations that represent the NACA 0012 airfoil, instead of the linear aerodynamic properties. For Case 7, unsteady aerodynamic terms were added to the model. Radial flow corrections were added for Case 8, and in Case 9 flap and lead-lag bending modes were incorporated in the model. Case 10 added the blade torsion modes and represented an "all-up" calculation. Note that three cases were included where the root cutout was increased from 0.228R, the value for the Puma, to a value that would exclude reversed flow for the sample problem (Cases 2, 4, and 6).

The high-speed case examined was based on a flight test condition for the research Puma with an advance ratio, μ, of 0.38; thrust coefficient over solidity, C_T/σ, of 0.08; shaft angle relative to the freestream, α_s, of –6.8 deg; first harmonic cosine flapping, β_{1c}, of 0.39 deg; and first harmonic sine flapping, β_{1s}, of –0.07 deg. However, the sample problem was for a rectangular blade, rather than the swept-tip blade of the research Puma. Four analytical methods were used to examine this problem. The Westland/DERA code, developed collaboratively by Westland Helicopters and the Defence Evaluation and Research Agency (DERA) in the United Kingdom, was used by Colin Young of DERA. The R85/METAR code, developed by Eurocopter France, was run by Francois Toulmay of Eurocopter. The CAMRAD/JA analysis was run by Thomas Maier of the Aeroflightdynamics Directorate, while the original CAMRAD analysis was used by Neil Gilbert of the Aeronautical and Maritime Research Laboratory (AMRL) in Australia.

The comparison of these calculations in Ref. 8 indicated that the effects of reversed flow, nonlinear and

Table 1. – Stepwise analytical model for high-speed rotor case. r_c is the radius of the root cutout and R is the rotor radius.

Case	r_c/R	Wake	Aero Tables	Unsteady Aero	Radial Flow	Bending Modes	Torsion Modes
1	0.228	uniform	constant	no	no	no	no
2	0.400	uniform	constant	no	no	no	no
3	0.228	prescribed	constant	no	no	no	no
4	0.400	prescribed	constant	no	no	no	no
5	0.228	prescribed	NACA 0012	no	no	no	no
6	0.400	prescribed	NACA 0012	no	no	no	no
7	0.228	prescribed	NACA 0012	yes	no	no	no
8	0.228	prescribed	NACA 0012	yes	yes	no	no
9	0.228	prescribed	NACA 0012	yes	yes	yes	no
10	0.228	prescribed	NACA 0012	yes	yes	yes	yes

unsteady aerodynamics, and radial flow had little effect on the blade vibratory loads. The important effects, by and large, were seen in comparing Cases 1, 3, 9, and 10. The calculations for vibratory normal force at 0.95R are shown in Figure 1 for the four calculations. There is no Case 10 calculation for R85/METAR as the analysis would not converge with the torsion degree of freedom added. The vibratory airloads are largely 3/rev because of the reduced or negative lift that occurs at the beginning of the second quadrant. Generally, the effect of the prescribed wake (Case 1 to 3) was to reduce the amplitude of the 3/rev loading, but increase the amplitudes of higher harmonics. The addition of bending modes (Case 9) and torsion modes (Case 10) tended to increase the harmonic loading.

The differences between the cases (or between the codes) for normal force can be assessed quantitatively using harmonic correlation (Ref. 9). In this approach one case is considered the reference condition or independent variable and the second case is considered the dependent condition. The sine and cosine harmonic coefficients of the dependent case are then plotted against the harmonic coefficients of the reference case for any range of harmonics. A least squares line is fitted to the resulting plot and the slope, *m*, and correlation coefficient, *r*, are computed. A slope close to one indicates very good agreement between the reference and dependent cases, and a correlation coefficient near one means there is very little scatter. This is illustrated in Figure 2 for the Westland/DERA calculations for four combinations. The first subfigure shows Case 1 as a function of Case 3. The solid circles represent the individual sine and cosine harmonics and the solid line is the least squares fit. The slope and correlation coefficient are indicated on the figure. Perfect correlation is indicated by the dashed line. For this comparison the calculated slope and correlation coefficient are largely dependent upon the 3rd and 4th harmonics of the lift. The effect of the prescribed wake for this calculation is to reduce the size of these lower harmonics, but to increase the size of the higher harmonics. In the second subfigure, Case 3 is compared to Case 9, that is, the effects of adding blade bending modes are examined.

In this case more scatter is seen in the comparison and the addition of elasticity has a significant effect on the phase of the harmonics. There is also, on average, an increase in the amplitudes. The additions of the torsion modes, Case 10, is illustrated in the third subfigure. The scatter is reduced in this case and there is also a reduction in the harmonic amplitudes. The final subfigure compares the full-up calculation, Case 10, with Case 1 and there is almost no correlation between the two calculations. This final result is not surprising, of course.

The differences in the stepwise calculations for the four analyses is summarized in Table 2 using the slope/correlation coefficient pairs for each comparison. It is interesting to note substantial difference between these methods for the various steps. The Westland/DERA, R85/METAR, and CAMRAD/JA codes all show a general reduction in the amplitude of the vibratory loads when the prescribed wake (Case 3) is added, but CAMRAD shows an increase in these loads. Similarly, when blade bending modes are included (Case 9), the vibratory loading increases for the Westland/DERA code, but decreases for the other three.

A cursory examination of Figure 1 suggests that each of these analyses is getting about the same vibratory loading. Yet when the calculations are examined in detail, it is seen that each analysis is showing differing effects of the wake, elasticity, and, perhaps, trim. In one sense the generally good qualitative agreement that is seen in Figure 1 represents today's state of the art–our best analyses provide reasonable results. But the comparison in Table 2

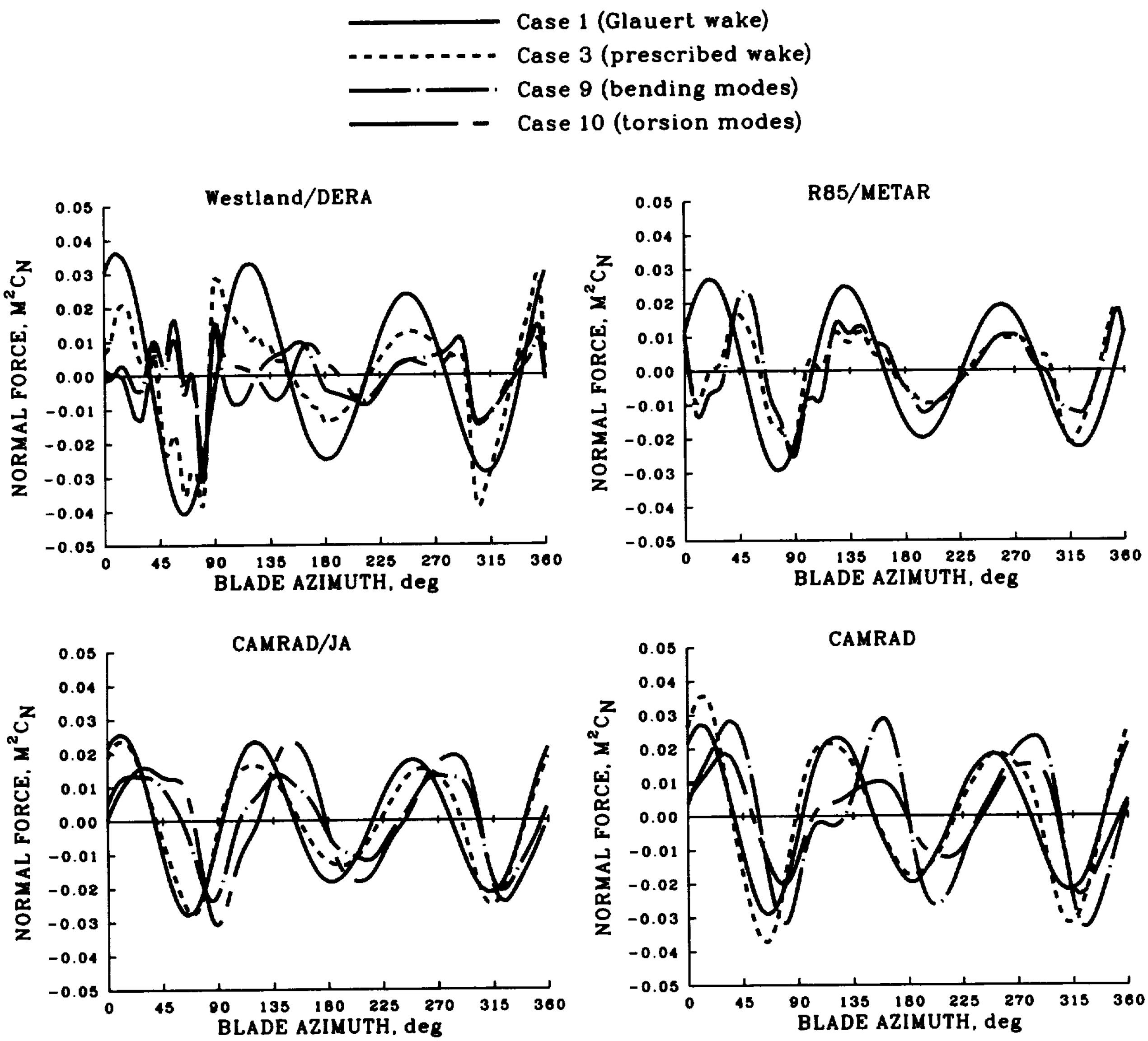

Figure 1. – Comparison of four analytical methods for stepwise calculation of normal force; r/R = 0.95, μ = 0.38, 3–36 harmonics. Section normal force coefficient, C_N, is multiplied by the Mach number squared.

shows a picture of significant differences between the methods. Some of these analyses suggest that the wake representation is what is most important; others that the blade elasticity is just as important or more so. Hidden in this comparison is the result of the trim solution. How much of the difference that is seen here is the result of differences in how the trim is obtained?

The experiments with the comprehensive codes shown here do not answer the question of what features are most important for the correct prediction of loads and vibration. Rather, the differences that are seen suggest that the correct answer will require that *all* features of the analysis be properly represented.

Vibratory Loading at Low and High Speeds

Typically, helicopters encounter their highest vibration in two different speed regimes. High vibratory loads are first encountered in the low-speed transition regime and then again at high speed. Depending upon the helicopter the transition vibratory loading may be more or less severe than the vibratory loading that builds up at high speed. The basic physics of the vibratory loading is of little importance if the primary objective is to control

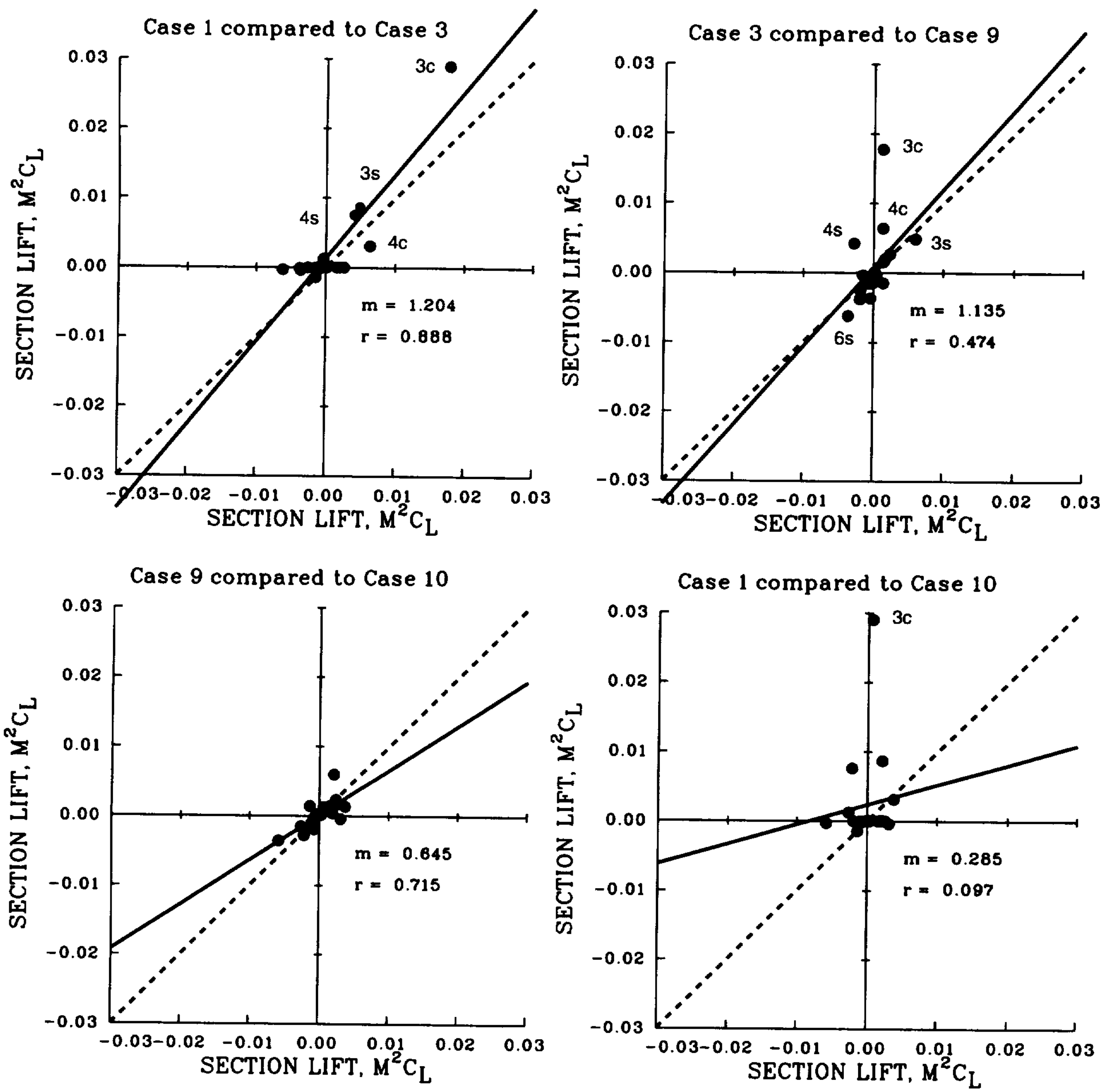

Figure 2. – Harmonic correlation showing stepwise comparisons between cases for Westland/DERA analysis; 3–12 harmonics.

Table 2. – Slope/correlation coefficient pairs from harmonic correlation for stepwise calculations; harmonics 3–12.

	Westland/DERA	R85/METAR	CAMRAD/JA	CAMRAD
Case 1/3	1.20/0.89	1.23/0.76	1.09/0.97	0.84/0.97
Case 3/9	1.14/0.47	0.84/0.90	0.74/0.60	0.86/0.53
Case 9/10	0.65/0.72	–	0.67/0.93	0.58/0.96
Case 1/10	0.29/0.10	0.88/0.58(1)	0.18/0.18	0.27/0.31

(1) Case 1/9.

the vibration with absorbers, isolators, or active controls. However, these physics become important if features of the blade design are to be used as a means of vibration control. The blade aerodynamics and the rotor response are very different in the two regimes and it is essential to keep these differences in mind in attempting to model and understand vibratory loads.

Figure 3 compares normal force measurements at 0.95R with calculations for two aircraft. The airload measurements for the UH–60A were obtained in 1993 (Ref. 10) and are interpolated from measurements at 0.92R and 0.965R, while the calculations are from Ref. 9. The research Puma measurements and calculations are from Ref. 9. The calculations were made using the CAMRAD/JA analysis with both prescribed wake and free wake models. At low speed the prescribed wake is unable to capture the rapid variation in aerodynamic loading, but the free wake model obtains significantly better results. At high speed there is no difference between the two wake models. For a conventional rotor the importance of the free wake declines as airspeed increases and little difference is seen between the wake models for advance ratios above 0.2 (Refs. 9, 11).

The importance of the free wake for vibration prediction in low-speed flight has been illustrated in Ref. 9 and figures from that study are repeated here. Figure 4 shows both a blade plane view and a disk plane view for a calculation for the research Puma at $\mu = 0.098$ for the prescribed wake calculation. The blade tip vortices appear as a sequence of epicycloids that overlap as seen in the disk plane view from an azimuth of about 60 to 85 deg. If the tip vortices are cut in the $\psi = 75$ deg plane, as shown in Figure 4a), it can be seen that the vortices are stacked sequentially (prescribed), one after the other. The effect of each tip vortex, therefore, is lessened as the vortex wake is convected beneath the plane of the rotor and the combined effects of these well-spaced tip vortices do not dominate the airloads. In the case of the free wake, as shown in Figure 5, the wake epicycloids appear much the same in the second quadrant, but in the first quadrant there is considerable distortion as the various tip vortices wrap around each other. The cutting plane at $\psi = 75$ deg shows that the nearest vortices to the blade tip are derived from the –3 and –4 blade passages, where the notation –3 refers to the tip vortex generated by the blade that passed roughly 270 deg prior to the sketch shown here. Note that the –1, –2, –5 blade vortices are not nearly as close to the blade for this cutting plane. The combined effect of these closely intertwined vortices creates the rapid airload variation that is observed in the first quadrant in Figure 3. A similar phenomenon is seen on the retreating side of the rotor. Thus, computation suggests that the dominant vibratory loading source at low speed is the intertwining of the rotor tip vortices that occurs near the outer edges of the rotor disk and that these pressure fluctuations are of the proper frequency content to cause the 4/rev vibratory loads.

At high speed the situation is different. The epicycloids that describe the tip vortex trajectories are spaced further apart and, as the rotor disk plane is tilted more forward, the tip vortices are convected more quickly away from the disk plane. The source of vibratory loading on the blade at high speed is the impulsive-like negative loading on the advancing side of the rotor that is seen in Figure 3. There may be secondary effects from the –1 blade tip vortex, particularly when a vortex of opposite sign is created over the area of negative loading as seen on the Black Hawk. Tung et al. (Ref. 12) have hypothesized that the small disturbance seen in the UH–60A airloads at about $\psi = 90°$ is a consequence of a reversed rotation vortex. Nonetheless, the overall effect on the vibratory loading at high speed of these secondary vortices appears small and it is essential to focus on the primary cause of high-speed vibration which is negative loading in the first and second quadrants.

A contrary view on the necessity of a free wake model at high speed is provided by Refs. 6 and 13. Hansford and Vorvald (Ref. 6) state that a free wake is required for the prediction of high-speed vibration, yet do not make direct comparisons between prescribed- and free-wake models. Wang (Ref. 13) also states that a free wake is necessary for a correct calculation of the vibratory airloads based on his work with the UMARC/S analysis. Although he speculates as to the cause of the differences, he does not provide any airload comparisons for the prescribed- and free-wake models. The importance of the free wake at high speed will only be understood when all the researchers involved approach this problem as one where hypotheses are either affirmed or refuted within the normal construct of the scientific method.

Low-Speed Vibration and BVI Noise Are Different Beasts

The very important problem of noise radiated from blade-vortex interactions (BVI) in descending flight has been studied extensively in recent years. Higher Harmonic Control (HHC) actuators have been used in wind tunnel tests of a model rotor (Ref. 14, 15) to demonstrate the substantial reductions in the BVI noise that are possible for these conditions. However, in some cases the vibration is increased for the controls that reduce noise. Tests have also been performed on full-scale helicopters and tiltrotors in a wind tunnel with Individual Blade Control (IBC). It has been shown that noise and vibration can both be reduced simultaneously with the addition of

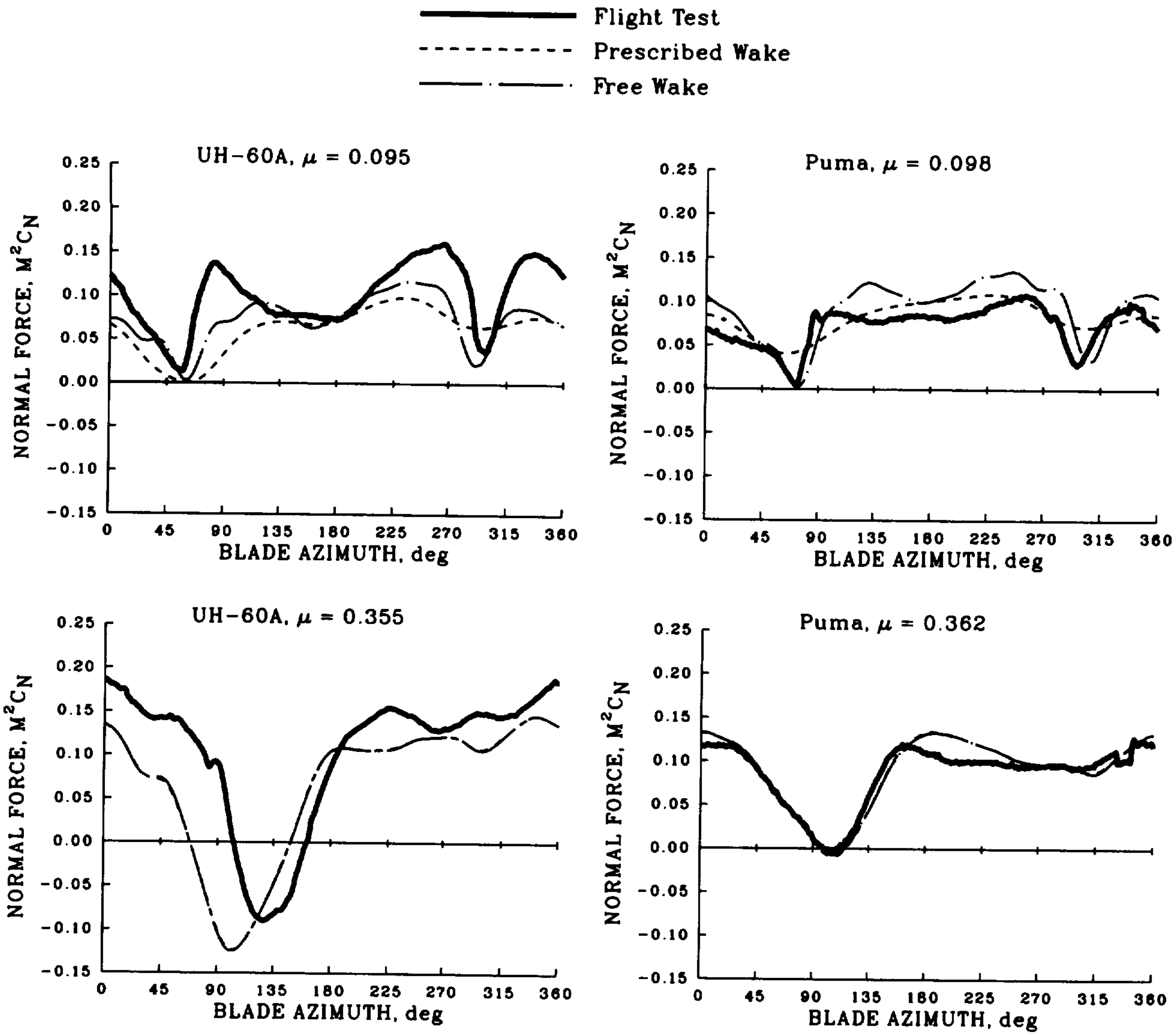

Figure 3. – Comparison of measured airloads for two aircraft and two flight speeds with predictions from CAMRAD/JA; r/R = 0.95.

2/rev control for a four-bladed helicopter rotor (Ref. 16, 17). Considerable effort has been taken to define the airspeed and descent conditions that result in the greatest BVI noise and it is these conditions that have been examined in the test programs. However, comparable efforts have not been made to identify maximum vibration conditions and, based on UH–60A flight tests, it appears that the maximum noise conditions encountered in flight tend to be the most benign in terms of vibration. This result suggests that these conditions are fundamentally not appropriate for testing closed-loop active controllers for vibration.

Crews has defined an Intrusion Index to represent how the pilot or crew respond to helicopter vibration (Ref. 18, 19). The Intrusion Index requires vibration measurements in three orthogonal axes for each location in the helicopter where the index is calculated. The three axes are weighted differently: the lateral or y-axis has an 0.75 weight relative to the vertical or z-axis and the longitudinal or x-axis has an 0.50 weight. In addition, each component includes a weighting factor that depends upon frequency. The Intrusion Index is the norm of the four largest harmonic amplitudes in each axis and, hence, is composed of 12 harmonic amplitudes. Although the 1/rev amplitude is excluded, all other harmonics up to 60 Hz are included and

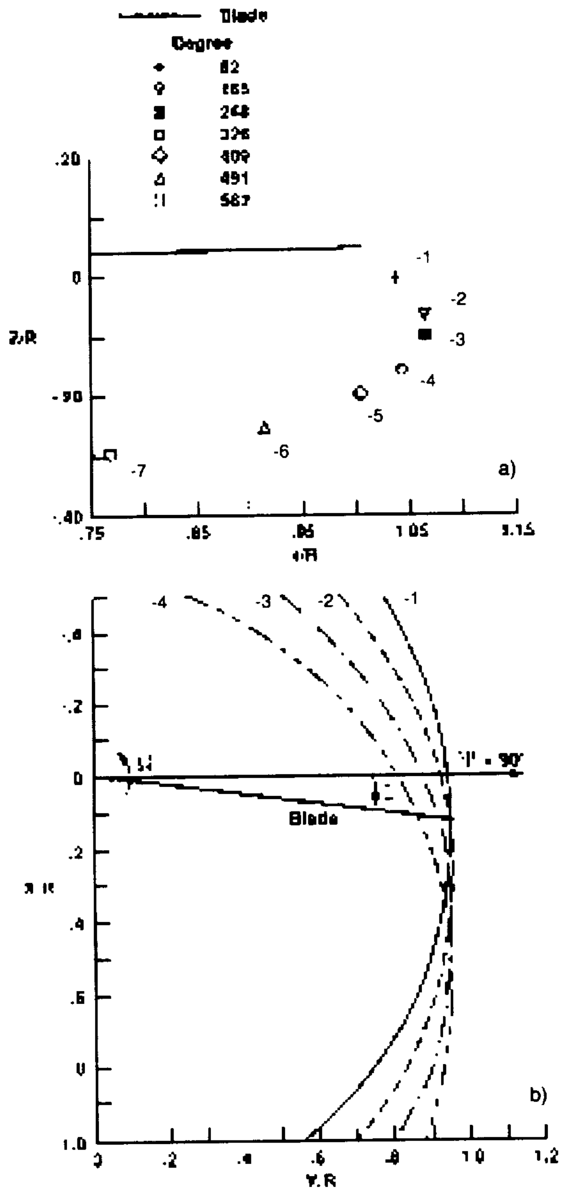

Figure 4. – Calculated tip vortex positions using a prescribed wake for the research Puma at μ = 0.098 (Ref. 9). a) View in cutting plane at ψ = 75°. b) View in disc plane.

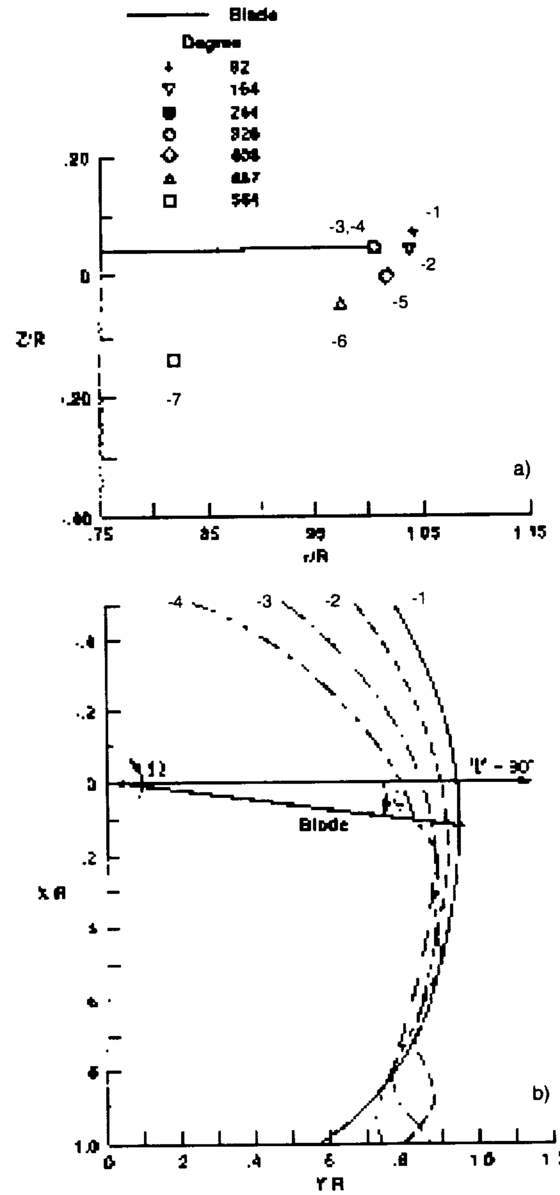

Figure 5. – Calculated tip vortex positions using a free wake for the research Puma at μ = 0.098 (Ref. 9). a) View in cutting plane at ψ = 75°. b) View in disc plane.

there is no restriction to include only *bN*/rev harmonics, where *b* is the blade number and *N* = 1,2, . . .

Figure 6 shows the Intrusion Index that has been computed for the pilot floor location in the UH–60A Airloads aircraft for a level-flight airspeed sweep at C_W/σ = 0.08, see Ref. 10. Data recorded by Crews (Ref. 18) for another UH–60A show reasonable agreement with the present results. Each test point from the airloads data includes either 19 or 20 revolutions and the index has been computed separately for each revolution to obtain statistics for the index. The index is relatively high in hover, about 1.5, and then drops below one at very low speeds. In the transition regime from about 0.08 to 0.10 advance ratio the index reaches an initial peak of about two. Above the transition regime the index declines to below one for advance ratios between 0.15 and 0.28. The index then increases and reaches two again at the maximum level flight speed.

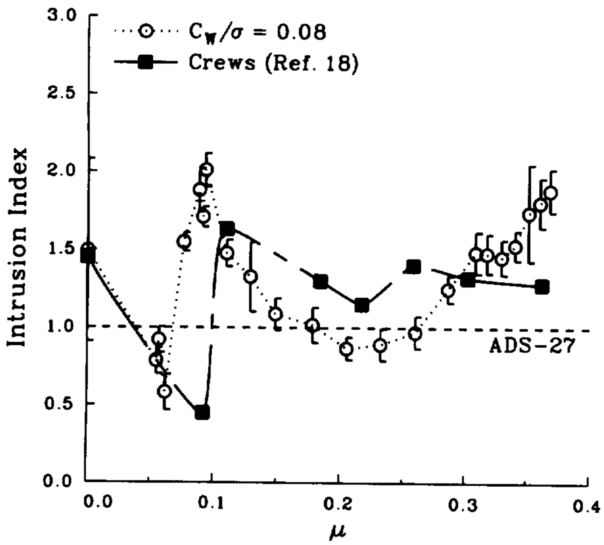

Figure 6. – Intrusion Index at pilot floor for UH–60A for level flight sweep at C_W/σ = 0.08 compared with Ref. 18.

It is expected that the norm of the Intrusion Index should be dominated by *bN*/rev harmonics and this is true for an index computed in the transition range. An examination of the norm at μ = 0.09 shows that the combined effect of 4/rev and 8/rev accounts for 91% of the index. However, at the maximum speed, μ = 0.37, the 4/rev harmonics account for only 67% of the index. The addition of the 2/rev vertical brings the norm to 86% and 6/rev vertical brings it to 91% and this demonstrates the importance of non-*bN*/rev harmonics for some flight conditions.

During the UH–60A Airloads program (Ref. 10) an extensive set of data was obtained using ground-acoustic measurements in cooperation with Langley Research Center (Ref. 20). These data included level flight, climbs, and descents relative to a microphone array installed on the ground. The Intrusion Index for the level flight cases that were flown during the ground-acoustic tests are compared in Figure 7 to the C_W/σ = 0.08 airspeed sweep data from Figure 6. For the ground acoustic tests, the aircraft was operated at a lower weight than for the airspeed sweep data and C_W/σ was about 0.065. As seen in Figure 7, the Intrusion Index is about 0.5 units higher for the ground-acoustic tests and the source of the increase is not known. Most of these data were obtained between advance ratios of 0.10 or 0.15 and 0.30 and this range of test conditions excludes the transition and high-speed vibratory range.

The greatest amount of data in both climbs and descents was obtained at μ = 0.15 and the Intrusion Indices for these cases are shown in Figure 8 as a function of the flight path angle. Flight path angle (or shaft angle in a wind tunnel test), has a relatively weak effect on

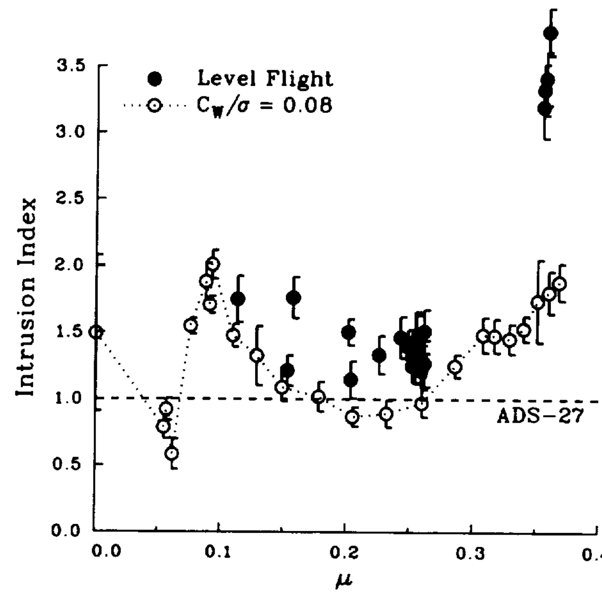

Figure 7. – Intrusion Index at the pilot floor for level flight ground-acoustic test conditions compared with C_W/σ = 0.08 airspeed sweep.

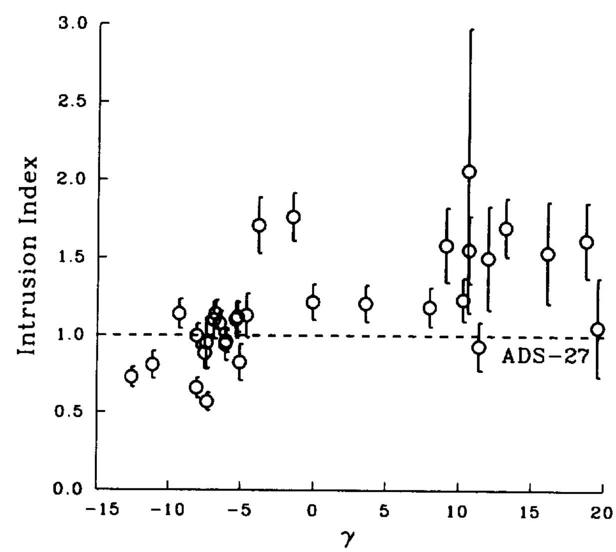

Figure 8. – Intrusion Index at pilot floor for various rates of climb and descent during ground-acoustic testing of UH–60A Airloads aircraft.

vibration, although for the higher rates of descent the index declines significantly. Although acoustic results for these cases have not been published, in general, the maximum noise is seen for descent angles of 5 to 10 deg and the least noise is radiated in climb. For instance, wind tunnel tests of a V–22 model (Ref. 22) show an approximate change of 20 db in BVI noise over a similar range of flight path angles. Thus, the greatest noise is

observed where the vibration is the lowest while the lowest noise is recorded where vibration is significantly increased. At low speed the phenomena involved in BVI noise are different than the basic phenomena involved in vibration. The noise is largely caused by parallel interactions of separate tip vortices shed from previous blades, while the vibration is a result of the intertwining of the tip vortices at the edge of the rotor disk. Thus, dynamicists need to focus their examinations of low-speed vibration not on descending flight, but on the speed regime for peak transition vibration.

Two Unsolved Problems

A number of significant aerodynamic load problems remain unsolved for articulated rotors and an advance is required in this area if any progress is to be made in the accuracy of our predictions for loads and vibration. These represent only single parts of a complex puzzle–nonetheless, they are a good place to start.

Negative Lift in High-Speed Flight

The airloads on the outer portion of the blade in high-speed flight are characterized by reduced lift at the end of the first quadrant and the beginning of the second quadrant, as is shown for the UH–60A and the research Puma in Figure 3. This negative loading starts to appear at μ = 0.25 or 0.30 and becomes progressively stronger as advance ratio increases. It appears to occur on all rotors for which measurements are available (Ref. 22, 23). Detailed comparisons of calculation methods with the research Puma data (Ref. 8, 24) show relatively good agreement with the measurements for this rotor. However, good agreement is not obtained for the UH–60A. Figure 9 shows a comparison from Ref. 25 of the measured section lift at 0.775R and 0.965R with the predictions of 2GCHAS and CAMRAD/JA. Although the amplitude of the negative lift appears approximately correct for both analyses, each shows a significant phase error. Lim (Ref. 26) has examined this problem by experimenting with options available in 2GCHAS, but has not found modeling changes that resolve the phase difference, although a number of the options changed the amplitude.

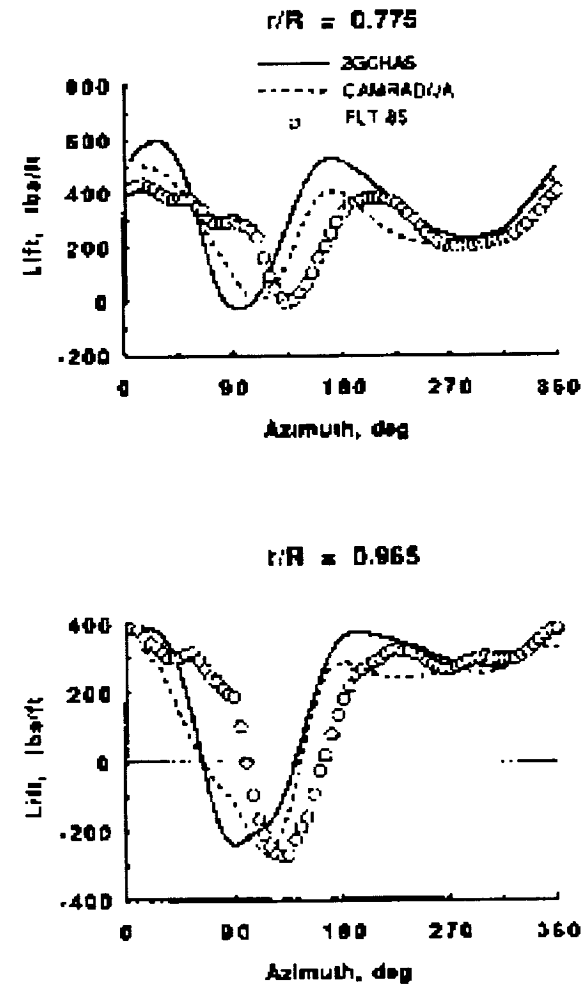

Figure 9. – UH–60A airloads measurements compared with 2GCHAS and CAMRAD/JA analyses for two radial stations; μ = 0.368 (Ref. 25).

The UH–60A model-scale data obtained in the DNW tunnel (Ref. 27) show the same phase as the flight data (Ref. 28) and Sikorsky has examined this problem by replacing portions of their analytical code with measurements (Ref. 29, 30). Using the measured airloads (Ref. 29), Torok and Goodman were able to show very good results for the phase of flap bending moments, suggesting that the calculation of the aerodynamic loading was the major problem. They then substituted the measured torsional deformation into the analysis instead of the measured airloads (Ref. 30) and in this case obtained much improved results for the phase. Thus, it appears that the problem is with calculation of the torsional response . . . if one believes in silver bullets.

Why is the calculation for negative lift for the research Puma satisfactory while the same analysis cannot predict the correct phase of the UH–60A? Both articulated rotors are of roughly the same size and both have swept tips (although the tip designs are quite different). Perhaps the single greatest difference between these two rotors is the blade twist. The Puma has about –8 deg of twist while

the UH–60A has twice that. Is twist, then, the important parameter?

Underprediction of Blade Pitching Moments

A second problem that is a significant barrier to progress in the prediction of loads and vibration is the inability of lifting-line analyses to predict the section pitching moments and hence the resulting control system loads. Figure 10 shows the oscillatory pitch-link loads as a function of advance ratio for the research Puma (Ref. 8) and the UH–60A (Ref. 31). All of the analyses shown consistently underpredict the oscillatory amplitudes over the entire speed range of airspeed. The detailed time histories for these calculations at high speed are shown in Figure 11. For the research Puma, the CAMRAD/JA and Westland/DERA analyses show reasonable agreement in the qualitative form of the time history, but underpredict the amplitude by about a factor of two. The original CAMRAD analysis, however, does not show the correct qualitative behavior. For the UH–60A the CAMRAD/JA analysis also fails to show the correct qualitative behavior. The inability of these analyses to predict the correct torsional loading means that the torsional deformation will also be incorrect and this will cause errors in the aerodynamic lift. There is no more fundamental problem in rotorcraft aeroelasticity than the deficiency in prediction illustrated here.

Maier and Bousman (Ref. 31) have examined the torsion loading problem in considerable detail, taking advantage of a number of experiments flown with the research Puma (Ref. 32, 33). Figure 12 compares the prediction and measurement of the two components of torsional loading for this swept-tip rotor at $\psi = 0$ deg. The section moment is just the aerodynamic moment about the local quarter chord, that is, the moment cause by location of the airfoil center of pressure. The flight measurements show that this moment in increasing as the blade tip is approached and CAMRAD/JA does not predict this effect. The lift-offset moment is a consequence of the section quarter chord being offset from the pitch axis of the blade. For the research Puma the quarter chord first sweeps forward and then aft. The CAMRAD/JA analysis shows a very good prediction of the section lift and hence the lift-offset moment. It appears that the prediction problem is largely one of calculating the unsteady aerodynamic moments near the blade tip.

Lifting-line analyses become less accurate close to the blade tip and it would appear that the torsion loading problem would be amenable to CFD techniques. Bauchau and Ahmad (Ref. 34) have coupled the Navier-Stokes OVERFLOW code to the DYMORE finite element model and examined a moderate speed case for the UH–60A. The results for lift are unsatisfactory and it is not possible to

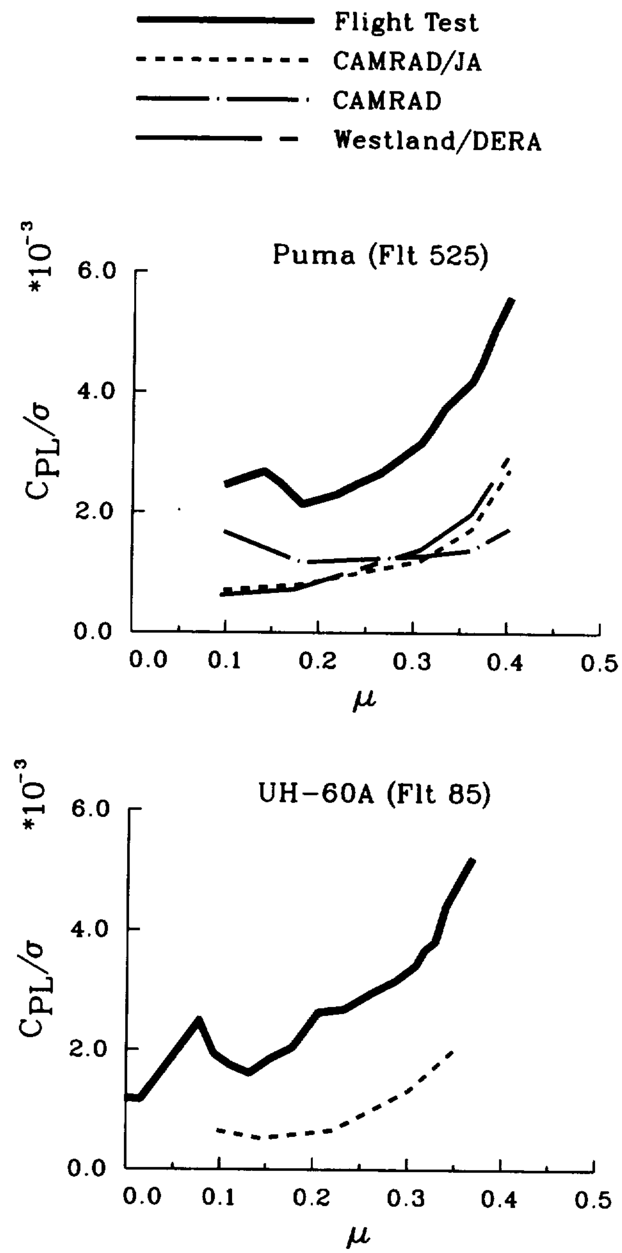

Figure 10. – Measured oscillatory pitch-link loads compared with analyses for the research Puma and the UH–60A; $C_W/\sigma = 0.08$, 1–12 harmonics.

judge the moment predictions as an error was made in the computation (Jasim Ahmad, *pers. comm.*).

Not all practitioners have failed at this problem, however. Pavlenko and Barinov (Ref. 35) have documented the design experience of the Mil bureau for four articulated rotors: the Mi–34 (3,400 lbs), the Mi–28 (27,900 lbs), the Mi–8 (30,400 lbs), and the Mi–26 (142,000 lbs). Figure 13 is a composite of various figures from Ref. 35 and shows the oscillatory torsion

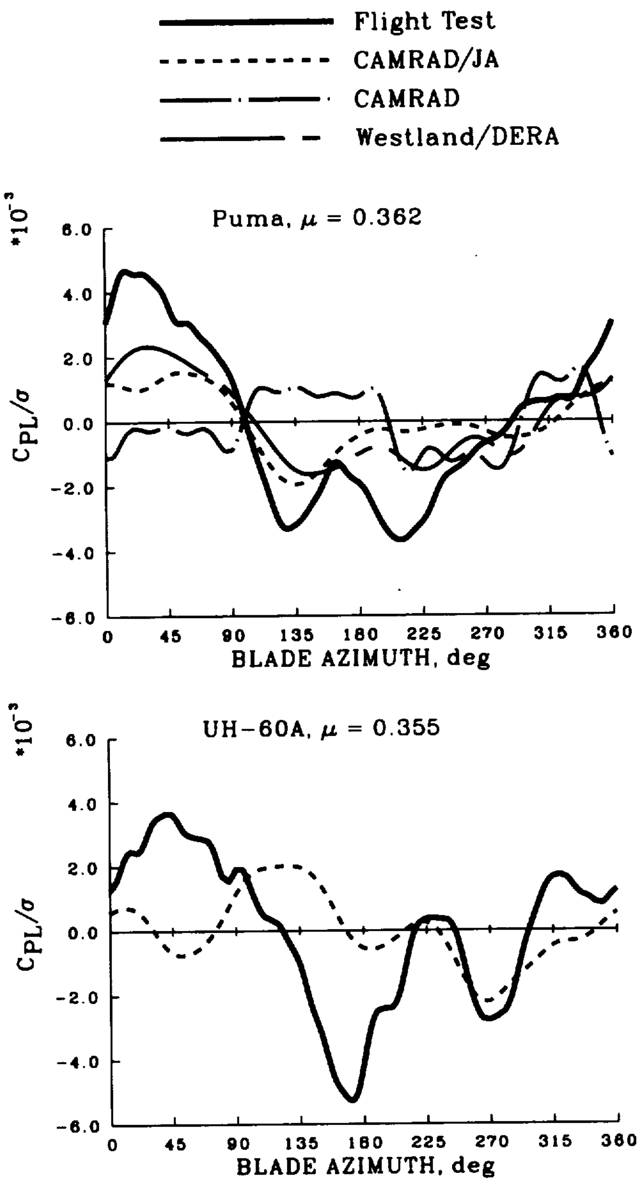

Figure 11. – Measured oscillatory pitch-link loads compared with analyses for the research Puma and the UH–60A at high speed; C_W/σ = 0.08, 1–12 harmonics.

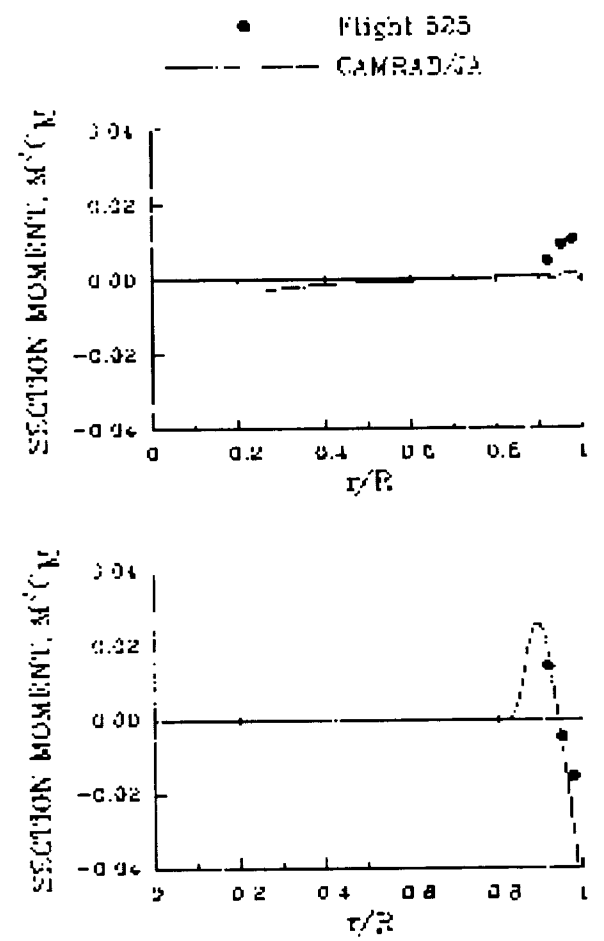

Figure 12. – Comparison of measurement and analysis for research Puma; ψ = 0 deg, μ = 0.402 (Ref. 31). a) section moment (relative to local quarter chord). b) lift-offset moment.

loads (half peak-to-peak) as a function of airspeed. Very good predictions are obtained for these oscillatory loads and the analysis appears adequate for design. Figure 14 shows the time histories of the torsion loads at high speed for the four aircraft. The Mi–34 data show one or more high frequency modes that are not predicted by the analysis and it appears that the loads are 180 deg out of phase with the other three rotors. The lower harmonics are well-predicted, however. The Mi–28 and the Mi–26 time histories are well predicted by the analysis. The Mi–8 waveform shows roughly a 35 deg phase error and is the least satisfactory prediction of the group. Nothing in Ref. 35 indicates that the Mil analysis extends beyond classical lifting-line theory, so their obvious success provides optimism that there is a near-term solution.

Cultural Barriers

A classical approach to problem solving in dealing with very complex systems is to reduce the problem to simpler parts, solve the simpler problems, and then integrate the simpler solution into the entire problem in the synthesis stage. The reductionist-synthesis approach is implicit in much of what we do as a community, but for a variety of reasons we have lost control of this process or maybe we've just abandoned it, leaving it for someone else to pick it up. As noted at the start of this paper, we have not made any significant progress in the

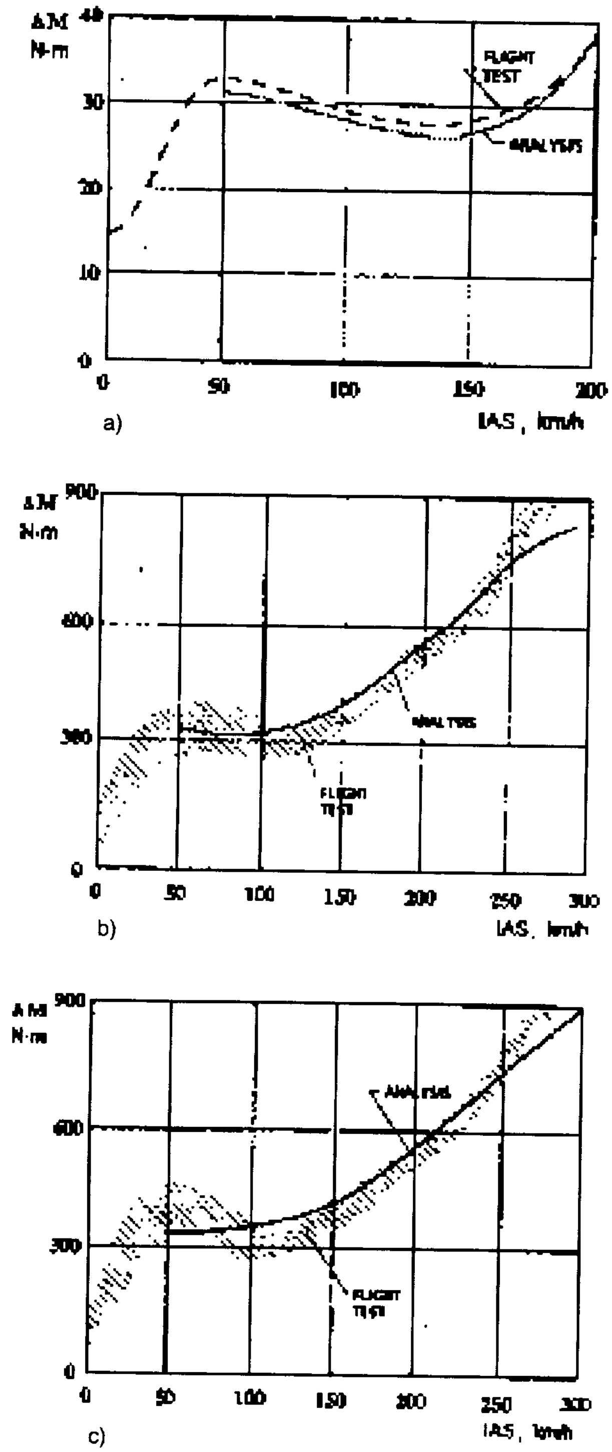

Figure 13. – Comparison of measurement and analysis of oscillatory torsional moment for three Russian aircraft: Mi–34, Mi–28 (rectangular tips), and Mi–28 (swept tips) (Ref. 35).

last 30 years in the accuracy of our prediction methods. If I would identify a single, most important reason for this lack of progress, then I would say it is simply the tremendous difficulty of the technical problems that we are dealing with. However, nearly as important, in my opinion, are some cultural barriers that we have allowed to develop. These barriers are so intimately tied into our culture, how we work, how we find resources for our work, and how we communicate that work that it is optimistic in the extreme to suggest that we can overcome these barriers, largely self-imposed, in the near future. Nonetheless, it is worth a try.

The cultural barriers I refer to are varied and closely interrelated. I will focus on three of these barriers that I believe have had the most deleterious effect on our progress and try to indicate changes that are needed. I will refer to the three barriers as (1) the need for humility, (2) the need to use quantitative evaluation metrics, and (3) the need to return to the scientific method. Each of these topics is so closely related to our own personalities and value systems as scientists that simply the process of listing the topics can be seen as an insult to the community. Nonetheless, arrogance intact, I will press on.

The Need for Humility

The dynamics community has dealt with the intractable problems of loads and vibration since the first helicopters and it does not appear that these problems have lessened in recent years. When one compares the multitude of papers that have been written in the last 30 years that include the words "for the first time" and the absence of papers that demonstrate improved prediction of loads and vibration for helicopters, one has to ask whether we have even the most basic understanding of what the objective of our research should be? Nothing can be more exciting for a scientist then to tackle a difficult problem, break it into manageable pieces, solve those pieces, and put it back together. Less exciting, but important nonetheless, is to work with one of those manageable pieces, and make real progress and share that with our technical peers. But we need to remember the purpose of what we are doing and that is to complete the synthesis, to bring the parts back together and demonstrate that they work. And that is where humility becomes important. We need to understand that our part is only one piece of the puzzle. We need to understand that those that have gone before have given us the framework from which we can build. We need to understand that the value of our work exists only in that it will be used and contribute to the whole.

When I suggest that our community lacks sufficient humility, I am not objecting to the personal demeanor of

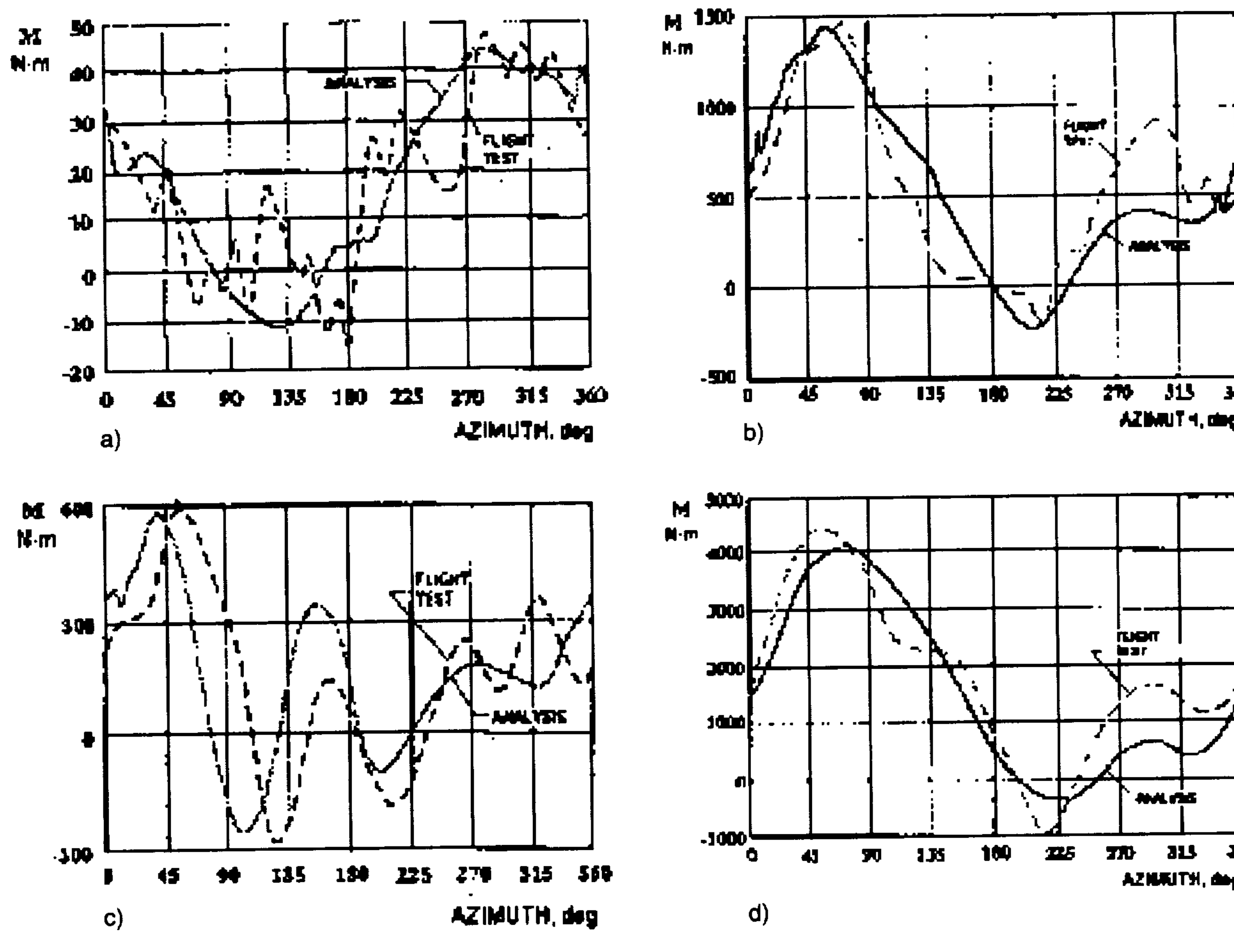

Figure 14. – Comparison of measurement and analysis of torsional moment for three Russian aircraft at high speed: Mi–34, Mi–28 (rectangular tips), Mi–8, and Mi–26 (Ref. 35).

researchers, but rather the words that they write. I understand that one cannot perform research without funds, and one cannot obtain funds without a proper representation of why the work is important. But when we turn this into a competition to see who can hype and exaggerate the importance of their research the most—and do this in our written papers, then we are harming our community. Let me give an example of what I consider a humble approach. Regrettably, I've taken this example from the CFD community; not from our own. In Figure 15 I show the flight test data, the prediction of a comprehensive analysis, CAMRAD/JA, and a coupled prediction between CAMRAD/JA and the Full-Potential Rotor (FPR) code. A number of features from this figure (and the paper) are important. First, although FPR can calculate pressure distributions on the blade and these were measured in flight test, this paper is not about pressure distributions but is about aerodynamic loading—features that are of primary interest to the rotor designer. Second, the figure compares the new results with data—it is important never to get too far from data. Third, the comparison compares the prediction of the new methodology with the current state-of-the-art, that is CAMRAD/JA in this case. The degree to which the new calculations advance the state-of-the-art is an issue that is clearly illuminated in this paper. Not all of us can easily compare our research with full vehicle measurements, nor can we always compare to the current technology. Our first objective, however, should always be to compare our new technology with measurement and comprehensive analysis and clearly illuminate what is the advantage we have brought to the state-of-the-art. And when it is not possible to make these comparisons, we need to qualify our conclusions and point out the work that remains before there can be a judgment of our new approach.

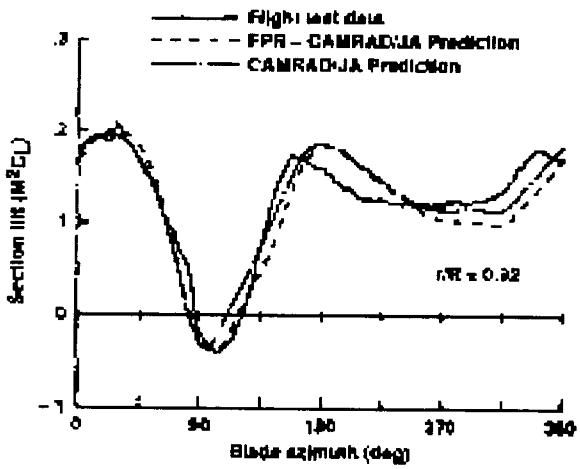

Figure 15. – Comparison of computational methods with measured airloads on research Puma (Ref. 36); μ = 0.38.

The Need for Quantitative Evaluation Metrics

Twenty-five years ago, at the Dynamic Specialists Meeting at Ames Research Center, Dick Bennett said ". . . correlation, like beauty, is in the eye of the beholder." (Ref. 37) In my opinion this statement has become a crutch for the dynamics community. Now, any conclusion about results, whether outrageous or simply vague, is considered valid because we know that correlation is not objective. Yet this statement has been taken out of context. Dick said these words as an example of fuzzy thinking and in the next sentence he stated "So we must come up with a good definition, a workable definition of correlation."

Perhaps we have progressed so little in the last 30 years because we are still waiting for someone to come up with a good definition of correlation . . . and are content to continue our subjectively-based conclusions until that day comes. I believe that we have failed to use quantitative metrics by choice, not because there is any difficulty in establishing these metrics.

As an example, in calculating hover performance, Kocurek et al (Ref. 38) compared their analysis with flight test data from 17 aircraft and defined an error band which provides a quantitative basis for judging the adequacy of their methodology. This approach was both systematic, in the use of so many data sets, as well as quantitative. Using a quantitative metric for a scalar value, such as hover gross weight may be simpler than establishing a metric for more complicated cases, but it was accomplished here because it was deemed important.

A first place to start for any quantitative metric is to plot the analytical results, in some form, as a function of the theoretical predictions. Good correlation is when everything lines up on a 45 deg line. I have illustrated harmonic correlation previously in this paper which is simply one method of applying this approach to the problem. But it is only one approach in a vast field of possibilities. The important point is not to select one, agreed-upon, quantitative definition of correlation for the entire community but, instead, always include quantitative metrics as a part of our research plans. Thus, before the research starts we need to identify the quantitative tests that will be made to assess how good or bad the results are.

The Need to Return to the Scientific Method

Every Philosophy Department in every university has their Social Constructionists and these folks occasionally talk eloquently about science and engineering. They accept no basis for either, but view them as social constructions of society. Thus, the scientific method is nothing more than a cultural artifact put together by a part of society for its own benefit. As for engineering, it is just a cut-and-try effort, accidental by and large, and again a vast set of rules or social constructs have been developed for the use of the practitioners. The first time one reads or listens to the exposition of a Social Constructionist one has the tendency to dismiss them as jargon-driven fools. But it his hard to avoid some of their conclusions, particularly with regard to such icons as the scientific method. And it is particularly hard to refute their claims if one is grounded in the literature of the dynamics community.

What is wrong, in dealing with a very difficult problem to propose a hypothesis, devise an experiment that will test the hypothesis, and then either confirm or deny the hypothesis? What is wrong with saying at the start, that a quantitative method will be used to confirm or refute the hypothesis? If there are no hypotheses to be tested and no metrics, then everything we do is just philosophy. Within the construct of the scientific method an "experiment" might, in fact, be theoretical development rather than a physical experiment. But this does not change the need to have the clear statement of the hypothesis and the means to test it. We admire researchers who bring to our problems their brilliance and all encompassing imaginations. Yet if their research is driven by ad hoc ideas that a new proposed functionality or a new scope for the analysis will at last provide the key to unlock the secrets of accurate prediction for loads and vibration, then I think we will face another thirty years absent of progress.

Is There a Way Forward?

I am hopeful that progress can be made in the next three decades and that this community will not have to listen to another frustrated researcher after another 30 years have past. Perhaps, real progress is near at hand, but my dim eyes cannot perceive this proximity. I think there are real criteria that we can use to help us and many of these criteria are used by practitioners today. In order of importance my list of criteria are:

1) Research tasks should be selected where it is clear how improvements, if proven, can be integrated in a synthesis step into one or more of our comprehensive analyses. Work on a reduced problem, if it cannot flow into the synthesis step, is of substantially less value.

2) Research tasks should be selected where there is an obvious reference or benchmark, preferably based on experimental measurements. Work should be discouraged for which there is no means of testing the improved analysis or knowledge.

3) The research proposal should always be clear as to how the improved analysis will be tested, that is, what quantitative metrics will be used to demonstrate success (or failure). Multiple metrics are suitable; the absence of metrics is not suitable.

4) Whether a researcher incorporates the result of their own research into the larger synthesis or leaves this task to others, it is essential that all necessary steps be detailed so that a competent scientist can duplicate the methodology.

5) Where a research program develops experimental data, either for the reduced program or the full synthesis, then these data must be made freely available to the research community.

6) All of the inputs and supporting data used for analysis should be freely shared.

There are a number of ramifications in my list of criteria and it is worth discussing these in detail. First, the openness that is inherent in some of these criteria, particularly the last three, eliminates participation in this larger process by the industrial scientist. Proprietary considerations, whether in respect to analytical or experimental results, are of great importance to the technical health and future prosperity of each of our industrial concerns. The dynamics community cannot expect industrial concerns to jeopardize their future potential for success. However, the industrial scientist does have an important contribution to make in a number of areas. For example, it is one thing to develop quantitative metrics, but in the end the specific numbers that are selected must be relevant to the industrial process. Within industry there is a great deal of design experience that is valuable to all of us in trying to determine how to establish quantitative metrics. We need to take advantage of this experience.

The last decade has seen the development of a number of comprehensive analyses that are no longer internal to just the companies. Thus, it is feasible to integrate new analyses into CAMRAD II, CAMRAD/JA, 2GCHAS, UMARC (and its various versions), CHARM, and FLIGHTLAB. A barrier to use of these analyses is the difficulty of establishing and checking out the input decks. Yet for many of the standard experimental problems a great deal of validation of the input data has already occurred and it is pointless to repeat this work. Thus, the free interchange of input decks is really a necessity if there is to be reasonable progress in the future. Eventually, it may not be too difficult to build input/output filters that can do most of the labor–intensive transformation between inputs for these various analyses. The more researchers that use any of these analyses the easier it will be to focus on deficiencies in input data and correct them. Research is always a competitive endeavor, but we should not be competing at these elementary levels.

Progress will probably occur only if the community is comparing multiple analyses over multiple data sets. Thus, to the extent data sets are restricted, the progress of the entire community is held back. In the experimental area, progress has meant a growth in the capability of accurate measurement as well as quantity of measurements. Thirty plus years ago, pressure data from ten conditions seemed sufficient from a test of the CH–34 rotor. All of these data were published in two slender volumes containing tables of harmonics or time histories (Ref. 39, 40). Nowadays, the increase in the number of parameters measured and the bandwidth that is used works against any publication of the data and we are faced with enormous problems of how to explore, understand, and share the data even in the absence of restrictions.

Just as the development and use of comprehensive analyses in the last decade has opened up new opportunities for the research community, it is apparent that new experimental data sets that are coming along will also provide a major opportunity to advance technology. Full-scale tests scheduled for the 40- By 80-Foot Wind Tunnel in the next five years may include the UH–60A, the AH–64A, and the V–22. Although only the first rotor will include pressure instrumentation, the presence of a dynamic balance on the Large Rotor Test Apparatus (LRTA) will provide unique vibratory load data that can be used to support research on vibratory loads. An expectation that useful data will be forthcoming to a wider

community is probably unrealistic. Unlike in the past when the publication of data was a mainstay of the Government research facilities, times have changed and more than likely the data will be "owned" by the stakeholders who have obtained it. Yet here also is an opportunity for the wider research community. Increasingly sophisticated measurements have become increasingly more difficult to make and there is the opportunity for individual researchers or consortia to demonstrate that they can enhance the value of these difficult experiments. At the same time they must negotiate aggressively concerning their stake in the resulting data.

What is needed in the future to advance the technology of loads and vibration is a strange beast indeed. One will need to be more open to opportunities to network with colleagues whether in industry, the Government, or academia. One will also have to have a good dose of paranoia to keep one's funding intact. One must be a true schizophrenic so that one can sell Mach 2, anti-gravity boots to the people with the money on one day, and the next day come to a forum such as this and tell the truth. Finally one must do good research. Ah, if it were only this easy.

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The lack of progress in understanding the physics of rotorcraft loads and vibration over the last 30 years is addressed in this paper. As befits this extraordinarily difficult problem, the reasons for the lack of progress are complicated and difficult to ascertain. It is proposed here that the difficulty lies within at least three areas: 1) a loss of perspective as to what are the key factors in rotor loads and vibration, 2) the overlooking of serious unsolved problems in the field, and 3) cultural barriers that impede progress. Some criteria are suggested for future research to provide a more concentrated focus on the problem.

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